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V. *Observations on the Foramina Thebesii of the Heart.* By  
Mr. John Abernethy, F. R. S. Communicated by Everard  
Home, Esq. F. R. S.

Read February 1, 1798.

As the investigation of the resources of nature in the animal œconomy, for the maintenance of health, and the prevention of disease, cannot but be interesting to the philosopher as well as to the physician, I therefore am induced to submit to the Society the following observations.

There is a remarkable contrivance in the blood vessels which supply the heart, not to be met with in any other part of the body, and which is of great use in the healthy functions of that organ, but which is particularly serviceable in preventing disease of a part so essential to life.

A distended state of the blood vessels must always impede their functions, and consequently be very detrimental to the health of the part which they supply; but, as the cavities of the heart are naturally receptacles of blood, a singular opportunity is afforded to its nutrient vessels, to relieve themselves when surcharged, by pouring a part of their contents into those cavities. Such appears to be the use of the foramina by which injections, thrown into the blood vessels of the heart, escape into the cavities of that organ; and which were first noticed by VIEUSSENS, but, being more expressly described by THEBESIUS, generally bear the name of the latter author.

Anatomists appear to have been much perplexed concerning these *foramina Thebesii*; even HALLER, SENAC, and ZINN, were sometimes unable to discover them; which suggested an idea, that when an injection was effused into the cavities of the heart, the vessels were torn, and that it did not escape through natural openings. When these foramina were injected, they were found under various circumstances, as to their size and situation; and HALLER observed, that the injection, for the most part, escaped into the right cavities of the heart. It also remains undetermined, whether these foramina belong both to the arteries and veins, or respectively to each set of vessels.

It is from an examination of these openings in diseased subjects, that a solution of such difficulties may probably be obtained. Whoever reflects on the circumstances under which the principal coronary vein terminates in the right auricle of the heart, will perceive that an impediment to the flow of blood through that vessel must occasionally take place; but the difficulty will be much increased, when the right side of the heart is more than ordinarily distended, in consequence of obstruction to the pulmonary circulation. Indeed it seems probable, that such an obstruction, by occasioning a distended state of the right side of the heart, and thus impeding the circulation in the nutrient vessels of that organ, would as necessarily occasion corresponding disease in it, as an obstruction to the circulation in the liver occasions disease in the other abdominal viscera, were it not for some preventing circumstances, which I now proceed to explain.

Having been attentive to some very bad cases of pulmonary consumption, from a desire to witness the effects of breathing medicated air in that complaint, I was led to a more particular

examination of the heart of those patients who died. In these cases, I found, that by throwing common coarse waxen injection into the arteries and veins of the heart, it readily flowed into the cavities of that organ; and that the left ventricle was injected in the first place, and most completely. When the ventricle was opened, and the effused injection removed, the foramina Thebesii appeared both numerous and large, and distended with the different coloured wax which had been impelled into the coronary arteries and veins. Upon eight comparative trials, made by injecting the vessels of hearts taken from subjects whose lungs were either much diseased, or in a perfectly sound state, I found, that in the former, common injection readily flowed, in the manner which I have described, into all the cavities of the heart, but principally into the left ventricle; whilst, in many of the latter, I could not impel the least quantity of such coarse injection into that cavity.

This difference in the facility with which the cavities of the heart can be injected from its nutrient vessels, was observed by most anatomists, though they did not advert to the circumstances on which it depended. HALLER's recital of his own observations, and of those of others on this subject, so well explain the facts which I have stated, that I shall take the liberty of quoting the passage, in order further to illustrate and authenticate them. He says, "Si per arterias liquorem injeceris, perinde in dextra auricula, sinuque et ventriculo dextro, et in sinu atque thalamo sinistro guttulæ exstillabunt; sæpe quidem absque mora, alias difficiliter, et nonnunquam omnino, uti continuo dicemus, et mihi, et SENACO, et clarissimo ZINNIO, nihil exsudavit."—*Elem. Physiol. Tom. I. page 382.*

As it seems right that the blood which had been distributed

by the coronary arteries, and which must have lost, in a greater or less degree, the properties of arterial blood, should not be mixed with the arterial blood which is to be distributed to every part of the body, but ought rather to be sent again to the lungs, in order that it may re-acquire those properties; we therefore perceive why, in a natural state of the heart, the principal foramina Thebesii are to be found in the *right* cavities of that organ. However, as, even in a state of health, those cavities are liable to be uncommonly distended, in consequence of muscular exertion sometimes forcing the venous blood into the heart faster than it can be transmitted through the lungs, there seems to arise a necessity for similar openings on the left side; but these, in their natural state, though capable of emitting blood, and of relieving the plethora of the coronary vessels, are not of sufficient size to give passage to common waxen injections. Yet, when there is a distended state of the right cavities of the heart, which is almost certainly occasioned by a diseased state of the lungs, these foramina leading into the left cavities then become enlarged, in the manner that has been already described; and thus the plethoric state of the nutrient vessels of the heart, and the consequent disease of that important organ, are prevented.

The preceding remarks will, I think, sufficiently explain the cause of the variety in the size and situation of these foramina, which also appear to belong both to the arteries and veins; because, the injection which was employed was too coarse to pass from one set of vessels to the other, and yet the different coloured injections passed into the cavities of the heart unmixed.

There is yet another mode by which diseases of the heart, that would otherwise so inevitably succeed to obstruction in the

pulmonary vessels, are avoided; and which I next beg leave to explain.

Having formerly been much surprised to find the heart so little affected, when the lungs were greatly diseased, and observing, in one or two instances, that the *foramen ovale* was open, I was led to pay more particular attention to the state of that part; and I have found this to be almost a constant occurrence in those subjects where pulmonary consumption had for some time existed previous to the person's decease. I took notice of this circumstance thirteen times in the course of one year; and, in several instances, the aperture was sufficiently large to admit of a finger being passed through it. Now, as the *septum auricularum* is almost constantly perfect in subjects whose lungs are healthy, I cannot but conclude, that the renewal of the *foramen ovale* is the effect of disease: nor will the opinion appear, on reflection, improbable; for the opening becomes closed by the membranous fold growing from one edge of it, till it overlaps the other, and their smooth surfaces being kept in close contact, by the pressure of the blood in the left auricle, they gradually grow together. But, should there be a deficiency of blood in the left auricle, and a redundance in the right, the pressure of the latter on this membranous partition, will so stretch and irritate the uniting medium, as to occasion its removal; and thus a renewal of the communication between the auricles will again take place.

From these observations it is natural to suppose, that in those men, or animals, who are accustomed to remain long under water, this opening will either be maintained or renewed: yet on this circumstance alone the continuance of their life does not depend; for we now have sufficient proof, that if the

blood is not oxygenated in the lungs, it is unfit to support the animal powers. There is an experiment related by BUFFON, the truth of which, I believe, has not been publicly controverted, and which tends greatly to misrepresent this subject. He says, that he caused a bitch to bring forth her puppies under warm water; that he suddenly removed them into a pail of warm milk; that he kept them immersed in the milk for more than half an hour; and that when they were taken out of it, all the three were alive. He then allowed them to respire about half an hour, and again immersed them in the warm milk, where they remained another half hour; and, when taken out, two were vigorous, but the third seemed to languish: this submersion was again repeated, without apparent injury to the animals.

This experiment is so directly contrary to what we are led to believe from all others, and also to the information derived from cases which frequently occur in the practice of midwifery, (in which, an interruption to the circulation through the umbilical chord occasions the death of the foetus,) as to make me suspect its truth: I was therefore induced to examine what would happen in a similar experiment. I did not indeed cause the bitch to bring forth her puppies in water; but immersed a puppy, shortly after its birth, under water which was of the animal temperature. It lost all power of supporting itself in about 60 seconds, and would shortly have perished, had I not removed it into the air. Neither could I, by repeating this experiment, so accustom the animal to the circulation of unoxygenated blood, as to lengthen the term of its existence in such an unnatural situation. I thought that a dog might have been made a good diver in this way; but, having satisfied myself

that this could not be done, without greatly torturing the animal, I did not choose to prosecute so cruel an experiment.

Young animals, indeed, retain their irritability for a considerable time, so that they move long after they have been plunged beneath water; and may even, on this account, recover after they are taken out. But the manner in which BUFFON has related his experiment seems to imply, that the circulation of the blood, and other functions of life, were continued after the animals had been excluded from the air. I am convinced that the poor dog who was the subject of my experiment would have been beyond recovery in a few minutes.

Those animals who are accustomed to remain long under water, probably first fill their lungs with air, which may, in a partial manner, oxygenate their blood during their submersion. The true statement of this subject may probably be, that the circulation of venous blood will destroy most animals in a very short space of time; but that custom may enable others to endure it, with very little change, for a longer period.